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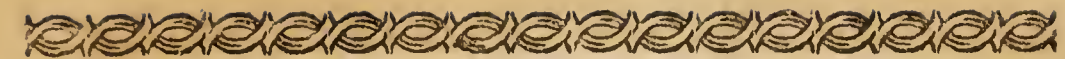
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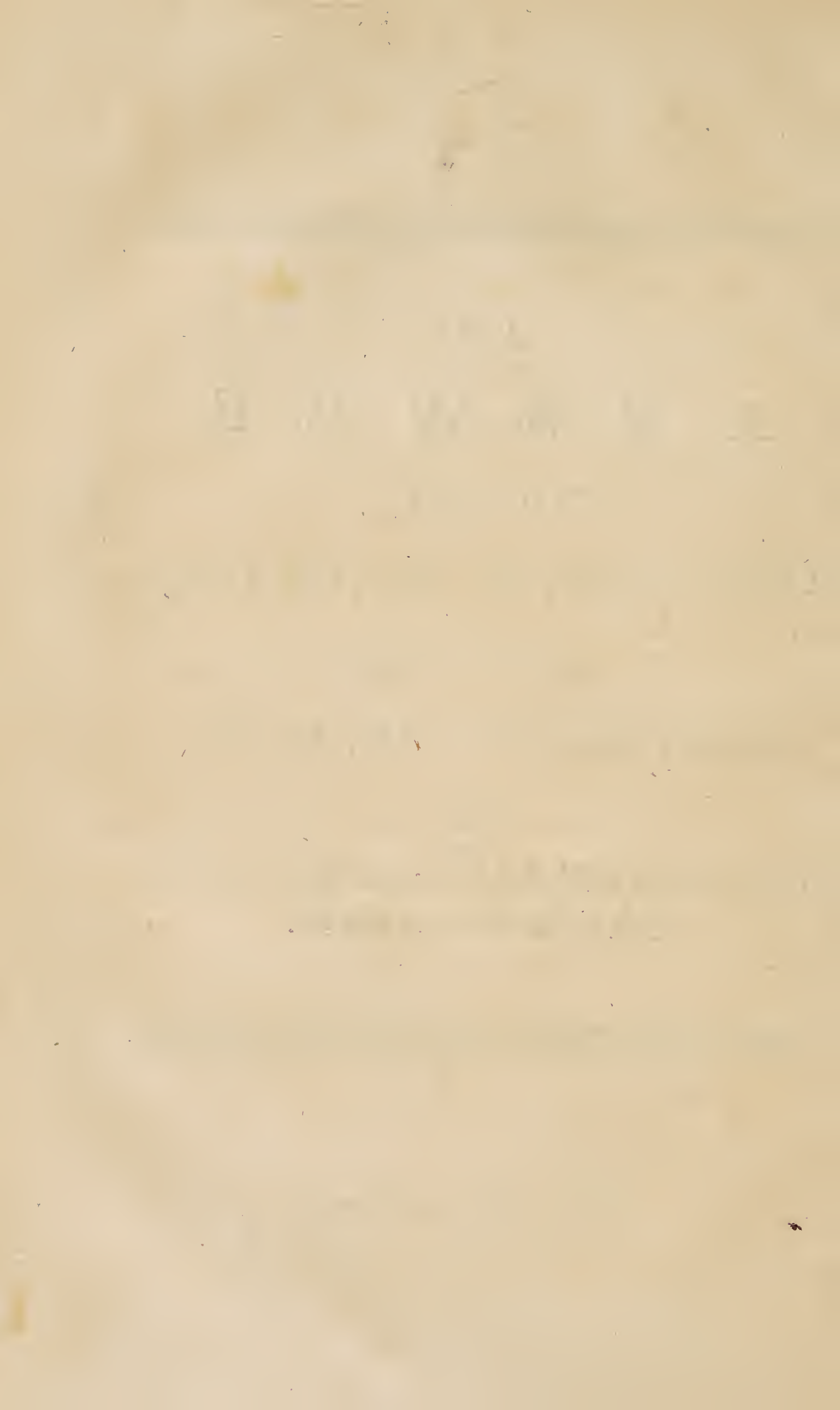
SAUNDERS, W.



A N
A N S W E R
T O T H E
Observations of Mr. GEACH,
A N D T O T H E
Curfory REMARKS of Mr. ALCOCK,
O N
Dr. BAKER'S ESSAY on the ENDEMIAL
COLIC of DEVONSHIRE.



[Price One Shilling.]



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O N
Dr. BAKER's ESSAY
O N T H E
ENDEMIAL COLIC of DEVONSHIRE.
In a LETTER from Dr. SAUNDERS
to Dr. BAKER.

Non persuadebis, etiamsi persuaseris.

L O N D O N:

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S I R,

I HAVE taken the earliest opportunity of perusing a Pamphlet, written professedly in opposition to the opinion, which is maintained in your Essay, concerning the cause of the endemial colic of Devonshire, by a formidable confederacy of Authors, Messrs. GEACH, and ALCOCK ; the former a Surgeon residing in the neighbourhood of Dr. HUXHAM, already well known to the medical world, on account of his new and curious *Observations on the Diseases of the Eye* ; the latter a Reverend Ecclesiastic, whose studies, as it may be presumed, have had but a small connection with medical subjects, and who has the merit of having been one of the first champions who lately appeared against the supreme authority in the cause of Devonshire Cyder. The former appears to me in the character of one who is desirous of neglecting no opportunity of paying every mark of his respect and adulation to a great name ; for his portion of this curious compound, contains little, very little more,

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than a dull, uninteresting, uninstruative repetition of a vain unsubstantial theory, together with an addition of much false reasoning, from his own private stock, and an abundance of misrepresentation.

Whether the latter has taken up his pen, upon a patriotic or a selfish principle, it cannot be easily determined; for although he appears to be a maker of cyder, and most probably is a dealer in that commodity, he must be allowed to have some share of public spirit, who seems to be as zealous “*to get your doctrine speedily refuted,*” as he lately was “*to get the cyder act repealed.*”

I will first examine the production of Mr. GEACH, after having premised, that I have very seldom met with observations more uncandid, or arguments less conclusive, than what here occur.

Indeed this man's arguments do every where appear to me to resemble those slender incorporeal beings, those *tenuēs sine corpore vitæ*, which the poet supposes to flutter about in the other world, *cava sub imagine formæ*.

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They are of too delicate and ethereal a structure, to bear the least touch, and he who makes an attack on them is in the case of a man, who draws his sword against a shadow.

Irruit & frustra ferro diverberat umbram.

“*Is it not more probable,*” says this acute reasoner, *that this colic is owing, in part at least, to a predominant acid, since other acids will occasion the same symptoms?*” How does this position appear to be true? Mr. GEACH proves his point in a most compendious manner, thus: “*such acid liquors undergo a new fermentation in the bowels; much hot elastic air is thus produced, causing all these spasms and colics.*” The substance of this argument is, that spasms and colics are caused by acids, because acids cause them. But it is undoubtedly true, that in the West Indies and America, they trust to the acids of lemons and limes, not only as the cure of, but as the preservative from the dry belly-ach.

It were to be wished that our ideas were not so grossly undetermined, with regard to the gross tartar, so much talked of as the cause of

this colic. No fruits in their unfermented state contain tartar of any kind ; it is generated only by fermentation ; and when properly separated from its inflammable matter, in the form of cream of tartar, as you justly observe, it becomes one of our safest and gentlest purgatives, and extremely well adapted to the cure of this disease.

Here follows a curious observation. “ *Air-bubbles are apt to get into all pipes and tubes that carry any fluid ; therefore no wonder if air-bubbles get into the pipes and tubes of the human body.*”

Another observation, as curious as the former. “ *It is probable that those severe pains, felt in the extremities, are for the most part flatulent ; since by rubbing the affected parts, the wind is often removed or expelled :*” From which of the extremities has this gentleman practised the art of expelling wind by rubbing ?

It is known by a common experiment made in the mechanical schools of philosophy, that water will transude through substances which are impermeable to air. No air can enter the
human

human body, or exist there, in a state of elasticity, nor can any thing restore elasticity to air, in the blood, but the taking off external pressure.

But, in case the existence of internal air-bubbles, in the vessels of the human body, should be controverted, Mr. GEACH has afterwards recourse to the external air, and he contends, that “*the disease must partly be owing to some peculiar disposition of the atmosphere.*” What is this, but an explanation of an *ignotum per ignotius*?

The state of the atmosphere has an undoubted influence on the human body; but I apprehend, an *untoward disposition* of the air, has often been unjustly condemned, as the source of diseases, in the production of which it has not been guilty. But, it is too common a fault of authors thus to build hypothetical castles in the air. They are apt to despise truth when it is near at hand, and can be obtained on easy terms; and are particularly delighted with the contemplation of objects, which are difficult of access, and placed at too great

great a distance to admit of perfect and distinct vision.

Having now examined how far air has been proved to be productive of this colic, let us next see how satisfactory Mr. GEACH accounts for it from vitiated bile.

If the disease, according to Mr. GEACH, depends "*on the bile's being exalted by heat ;*" ought it not to be more severe in warm autumns ? but he himself says, in page 4. that "*warm autumns are less productive of this disease than cold and inclement seasons.*" This gentleman further tells us, "*that the bile exalted by heat produces porraceous vomitings, so corrosive as to excoriate the mouth ; that in consequence of this fluid, wind and spasms are engendered, which bring on an obstinate costiveness ;*" yet in the very next page he tells us, "*that warm autumns,*" which according to his own system ought to be most effectual in exalting the bile, "*produce Diarrhœas and Choleras ; whereas in the colder seasons, obstinate constipations and rheumatisms prevail.*" A very consistent doctrine!

If the porraceous bile be so acrid, as Mr. GEACH represents it, how comes it to pass, that an obstinate costiveness is the consequence? would not a contrary disposition of the bowels naturally be expected from so acrimonious a fluid?

In another part of this work we find it asserted, that, "*bile thickened by the intemperate use of acids, loses its saponaceous quality, and becomes so corrosive as even to fret metals.*" Is then the bile more corrosive, because it is become thicker? Unfortunately for Mr. GEACH, he has here contradicted Dr. HUXHAM, who, in his treatise on the Devonshire colic, p. 22. has informed us, that, "*when the bile is mixed with too much acid, its powers are destroyed, it is rendered quite inert, and turned almost into a coagulum;*" Mr. GEACH will have a difficult task to explain how the same cause can render the bile more *inert*, and more *corrosive*: will he give up his own theory or that of his master's? What a cruel dilemma!

If the colica pictonum be produced by acids, a constipation of the bowels ought not to be a pathognomonic symptom of that disease. For
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it is proved by the experiments of Dr. RAMSAY, that acids render the bile more purgative. And in children this is sufficiently evinced, from the usual effect which is produced when acid prevails in the primæ viæ.

In p. 3. we read, that “ *the urine and serum of the blood is green, and the sweat sour.*” The existence of acid in the blood is by no means proved by any one. VAN SWIETEN speaks of acid sweats attending miliary eruptions; but in this he only refers to the sense of smelling, a guide extremely fallacious; and that acid sweats, as Mr. GEACH says, should prevail in autumn, while every symptom of alkalious bile shews itself, is too improbable to meet with acquiescence from any one. I wish he had determined this matter by his taste, that he might have offered us some specimens of his accuracy even in that sense.

“ *Ladies who drink but little cyder, and children who drink none at all, are often troubled with colic.*” This observation holds good all over the world; but proves nothing in the present case. For is every colic incident to ladies and

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children

children, the colic of Poitou? Are there no pathognomonic symptoms which distinguish this from every other colic? Besides, ladies of a tender habit of nerves, are known to be easily affected with a very small quantity of lead, even when externally applied.

“When the disease does not shew itself in severe colics, the patient is troubled with diarrhœa and cholera.” Now, such evacuations, generally prove critical in the colica pictonum; being attended with a ready discharge of the poison, which would otherwise be accumulated.

I believe no one, the author excepted, will conclude from the unimportant circumstances here enumerated, that *“we have the strongest reason to believe that this disorder cannot proceed from lead.”*

I am sure it has not been proved by Mr. GEACH, that, according to his first position, *“other acids devoid of all lead, taken in when new, and in larger quantities, than can be subdued by the vital force of the stomach, will occasion the same severe symptoms.”*

Further, it may be observed, “*that warm autumns, which ripen the fruits, are less productive of this disorder than cold and inclement seasons.*” And Mr. ALCOCK afterwards repeats, “*the distemper is observed to rage most in great bearings, and cold moist seasons.*” From the testimony of both, may we not conclude in favour of your opinion? For if lead by any means comes in contact with cyder, more of that poisonous metal will be dissolved in cold and inclement, than in mild and warm autumns; for the acidity will be greater. And hence it is easily seen why “*the colics which happen in mild and warm autumns, are hardly to be distinguished from ordinary diarrhœas.*”

It is likewise seen why the cause is not *universally* the same, and is therefore not likely to produce the “*same universal effect.*”

In p. 5. he speaks very learnedly of the “*sulphur of fruits,*” quite a new principle, and not to be understood by any one, not even by himself. I wish he had brought some proofs of its presence, and in what state it exists.

Page 7. "CITOIS calling this disorder, *novus popularis dolor colicus biliosus*, hints that it was epidemical, and concludes that the bile is concerned in it." To be sure if CITOIS calls this disease *popularis*, he does give a pretty broad hint, that it was epidemical; and it is certain that CITOIS, as a systematic writer, does mention the bile, as one of the internal causes of the disease; but I strongly suspect that Mr. GEACH knows no more of the treatise of CITOIS, than what he collected from your essay. Before he publishes another pamphlet, I advise him to read that author carefully; for he will find in him a great deal of matter worthy to be adopted by him. He will there find much said on the *bilis atra et æruginosa, porraceæ, perusta & præassata*. He will there also find quotations from HIPPOCRATES and GALEN, writers, certainly infallible in their physiology. To be serious, some allowance may be made for an Author of the year 1617, who might think it a *crimen læsæ majestatis*, to dare to dissent from the ancients. But what excuse can we make for writers, who in the present improved state of natural knowledge, continue to follow the steps of such blind guides, as the ancients undoubtedly, are in

almost every thing relating to the animal œconomy?

What Mr. GEACH says of the West India colic is directly contrary to truth. The West Indians, instead of the strong punch, which they formerly drank, drink it now very weak, and extremely acid. This I have received from the best authority.

“The Turks make their Sherbet with a small portion of acid.” I have received an account from a person, who resided in the East among the Turks many years, that their Sherbet is extremely acid. But let me caution Mr. GEACH, when he says that, the *“Turkish Sherbet differs very much from an acid fermenting juice,”* to recollect what the opinion of Dr. HUXHAM is, with respect to the cause of the dry belly-ach of the West Indies, viz. that it arises from a cause similar to that of the Devonshire colic, i. e. too great a use of the very acid juice of lemons.

I would likewise ask Mr. GEACH, whether he does not suppose that the people of Devonshire, as well as the Turks, drink their acid
liquor

liquor for *pleasure* ; and whether it is not reasonable to imagine, that the Turks are more likely, than the Devonshire people, to drink their liquor at a time, when the body is *overbeated by labour*.

Dr. WALL happened to say, that he had heard from a physician, that there were more lunatics in Herefordshire, than in the adjacent counties. He does not attempt to vouch for the fact, but only says, that the fact, if true, may possibly arise from the quantity of cyder drank, rather than the quality. This accidental intimation is to Mr. GEACH a strong confirmation that Dr. HUXHAM's assertion is well founded ; and he triumphs in the discovery. But if we grant it to be an undoubted fact, that Hereford cyder brings on lunacy, will it follow that lunacy and the colic of Devonshire are in the least analogous ? The effect of Devonshire cyder is palsy, the effect of Herefordshire cyder is lunacy ; the former is a diminution or total loss of the muscular power, the latter is often attended with the highest exertion of that power, which is in that disease generally entire and unimpaired. But I for once agree with Mr. GEACH in
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sentiment, “ *that the loss of strength is not so deplorable as the loss of understanding.*”

This reasoning is not confirmed “ *by observing that children become convulsed from acids abounding in the stomach;*” for by the same reasoning Mr. GEACH may prove, that the breeding of teeth, worms, and divers other causes, produce the same disease.

“ *The rickets are undoubtedly produced by acid in the blood and juices.*” What! because “ *sour milk and vinegar soften the cartilaginous and tender bones of infants, and sometimes even of adults,*” that is, I presume, of dead infants, and dead adults, does it necessarily follow that the same effects will take place in the living body? No acid, as I before observed, is to be found in the blood; and therefore I will boldly say, “ *that the rickets are undoubtedly*” not “ *produced by acid.*”

The next paragraph gives us a new cure for the rheumatism, which is likewise ascribed to acid. We are here taught that mild beer is an anti-acid, and that either that, or cold water, another anti-acid, ~~gives us~~ ^{cures} that disorder.

In your pamphlet, you say, that Jockeys drink largely of vinegar ; Mr. GEACH modestly answers, “ *this is not probable ;*” for, in his opinion, “ *the most robust would soon find it attended with very bad consequences.*” A very dogmatical and peremptory answer !

It avails nothing to prove, that girls have lost their health by drinking vinegar, unless it be proved, that it has brought on this particular disease ; and this may serve as a reply to the answer given by Mr. GEACH to your argument, concerning chlorotic girls. That a physician may have cured such a patient by means of the cold bath, any body may believe ; but if Dr. HUXHAM used no better anti-acid than *mild beer*, it will be reasonably suspected that the principal part of the cure depended on the cold bathing.

Page 12. Mr. GEACH has met with wrong information with respect to the people whom he calls *Danglers*. These are to be found, according to my information, not in the counties of Hereford, Gloucester, and Worcester, but in Somersetshire, and in those parts of Dorsetshire, which are near Somerset,
where

where I hear the Devonshire error of cyder-making prevails.

Dr. NOOTH assures me, that in Dorsetshire, particularly on the side near to Somersetshire, the cyder-presses are very frequently lined with lead; and that the colica pictonum is most frequent in those parishes where they use the most lead: he knows a gentleman who makes a great deal of cyder, but who uses no lead in his utensils; the colic of Poitou is never in that gentleman's family, although his servants drink large quantities of cyder.

It is likewise a common practice among the farmers of Dorsetshire, to buy from the apothecaries large quantities of *Sachar. Saturni*, with which they are known to sweeten their cyder. This I can affirm from the positive testimony of gentlemen of credit, who reside in that county.

A French writer is quoted by you to show, that Rhenish and Moselle wines occasion the colic, by the means of an adulteration. The passage referred to in your Essay is as follows: *Ces vins pechent souvent par trop de verdeur; et*
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les marchands les ont long tems alteré avec la litarge. But Mr. GEACH attempts to prove, from the authority of this very passage, that Rhenish and Moselle wines produce the colic, by means of their acidity. In order to do this he quotes only the first part of the sentence, *ces vins pechent souvent par trop de verdeur.* These words he construes thus, “ *these wines often hurt by their too great acidity*; and he entirely drops the subsequent part, which puts the Author’s meaning out of all possibility of doubt. On the strength of this he triumphs; “ *here we see that the mischief proceeds not from adulteration but the sourness.*” Every one moderately conversant with the French language, will at once see, that the word *pechent* is most erroneously construed *hurt*: the true construction of the sentence is, *these wines are faulty by means of their acidity; and the merchants have long adulterated them with litharge.* Upon no better foundation than this, Mr. GEACH ventures to affirm, “ *that these wines do in an unadulterated state bring on the colic, and we need only have recourse to Mr. BOUVART’S words quoted by Dr. BAKER, in order to prove it.*” It is curious to find afterwards that the Reverend Author of the Supplement does, with respect to the use made of this quotation,

neither *vary* nor *differ* from his *learned* friend. Indeed the *thoughts* of your two opponents do *entirely coincide* in this particular. By what name shall I call such a criticism? There appears to be in it, too evident marks of ignorance, for us, to suspect mere wilful misrepresentation; and at the same time there appears too evident marks of a wilful misrepresentation, for us, to suspect only ignorance.

Page 13. You have not asserted that the farmers of Devonshire use litharge; why are they defended against a charge not brought against them? I have however heard of a certain white powder called *pearl*, which has been thrown into cyder, and which has been supposed much more efficacious in sweetning it, than either treacle, or burnt sugar. And I believe, it may be truly asserted, that such a powder, which is a secret, has been used for the purpose of restraining fermentation; and that this is not an uncommon way of improving cyder.

“ *If Cerusse was thrown into cyder, the longer it remains there, the more impregnated would the liquor be.*” You have not said that Cerusse was thrown into cyder. Here is another

another defence where there was no accusation. *Nescio quid peccati habet hæc purgatio.* Every pupil in chemistry knows that the acid in cyder, is the only part of it, capable of dissolving this poisonous metal. The longer the cyder stands, if properly kept, it becomes the more mellow, *i. e.* it deposits its tartarous acid ; and the poison will consequently in part be precipitated along with it.

Page 14. Mr. WORTH is introduced, declaring, that “ *he never knew any lead used in any of the pounds, and yet that his tenants, and neighbours, are more universally afflicted with the colic, than any other people in other parts of the county.*” This is indeed a very general assertion ; but it is not probable, that a gentleman should have inquired into the state of the pounds, or into the state of the disease in the various parts of a large county, with sufficient accuracy, to enable him to give an evidence on this subject, which can in any degree be decisive. As to Mr. WARD’s two men ; had they drank 22 gallons of cyder instead of 22 quarts, adulterated with lead, without being hurt, such an observation would by no means prove, that a solution of lead

may in general be drunk with impunity. It is well known that a very small quantity will in particular constitutions do great and irreparable mischief, and that in other constitutions, a large dose will do no injury. Besides, it is a known fact, that some deadly poisons taken in too large a dose, will, by overacting their parts, even elude their own powers.

Page 15. “ *Not one pound in fifty contains any lead at all.*” Mr. GEACH, conscious that this is much too bold an assertion, or in other words absolutely false, qualifies it with, “ *or not of any consequence. All the apples are compressed in wooden pounds, or ground in iron or wooden mills.*” If all the apples are thus compressed and ground, it is I imagine to be presumed, that the Moorstone troughs, “ *in which a little lead is sometimes used,*” are never employed. To what shifts and contradictions is a man driven, who knows not, how to suppress truth with dexterity !

“ *It cannot I believe be proved, that sheet-lead is nailed over any press in any part of Devon, that at Alphington excepted.*” What will this Author say, if I reply, it can be proved, that in
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the year 1766, there were many such presses, in several parts of the county. It can be proved by a certificate, which you have shown me, that in three parishes only, there were at least thirty presses lined with lead. *This is no hearsay or random assertion.*

Page 16. “*The cramps fastened with lead, are seldom in the groove, where the apples are ground.*” For *seldom* read *always*. And this Mr. ALCOCK does not deny, but only attempts to be jocular on the subject.

“*Did corrosive vinegar instead of cyder flow from the pounds, could it possibly extract the ill qualities of the lead as it runs?*” In page 27 of your Essay, Mr. GEACH will find this quotation answered by Dr. WALL, who says, that he has lately had two or three patients in that distemper, occasioned by their having drank cyder made in a press covered over with lead. Besides, the circumstance to which he refers, of fresh surfaces of cyder, being so repeatedly applied; that, and the agitation of the must, during its running off, will necessarily facilitate and promote the solution of the lead.

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As to the quantity of lead used in the pounds, that is different in different places; and it depends very much on accident.

According to Mr. GEACH, "*the original quantity first used does not exceed four or five pounds;*" according to Mr. ALCOCK, "*two or three pounds are the quantity first used;*" but according to other authorities, ten, nay twenty pounds are employed. An argument founded on principles so vague, cannot but be fallacious and inconclusive.

Answer to Q. 1. It may be allowed that there is a possibility that some of the many patients said to have been received into the hospital at Exeter, may have drunk cyder made in pounds in which there is no lead; this I say may be allowed; and yet perhaps, it may hereafter appear probable likewise, that the cyder which they drunk was impregnated with lead.

Answer to Q. 2. Dr. HUXHAM's opinion is, that the Devonshire colic and the colic of the West Indies arise from *similar causes*, the first from acid cyder, and the latter from too great a use of the very acid juice of lemons.

And I would advise Mr. GEACH, if he would experimentally prove his own reasoning, to try the effects of cyder *taken occasionally, and in small quantities, for the cure of the gripes.* As to the bile, if it be true, that at Plymouth, when putrid, it becomes an *acrimonious corrosive sanies*, he may try the experiment on it, even with greater advantage at Plymouth than in the West Indies, where I believe its *sanies* was never quite so corrosive.

Answer to Q. 3. With respect to Mr. ROWE the rope-maker, who Mr. GEACH roundly says “*never drank any cyder,*” I must here suspend my belief. But I will admit that Mr. ROWE prefers white ale to cyder. No judgment however can be formed with regard to the positive effects of that liquor, till Mr. GEACH shall discover the secret of making white ale. That the composition is kept a secret, he will not deny. That if it occasions this identical colic, it does not occasion it by being in the act of fermentation, appears probable from the experience of the inhabitants of Scotland, who drink large quantities of a small beer while fermenting, without being affected with the colic of Poitou.

Answer

Answer to Q. 4. The experiments made on the cyder which flowed from the leaden pipes at Alphington, were very fair; however, they were by no means confined to such cyder; and almost all the varieties of Devonshire cyder, which you could procure, exhibited more or less the appearance of lead on the addition of the test. That the must when brought to London was not black, I can testify; nor was any specimen of cyder, on which you tried your experiments, of any but the common colour.

Answer to Q. 5. which runs thus; “*How comes it to pass that cyder, some thousand hogsheads of which are sent every year to London, does not produce the colic among the inhabitants there?*” Though much cyder be brought to London, it is very far from being the common drink of the inhabitants. And if we even suppose, that when it was exported from Devon, it all had a Saturnine impregnation, it would, by being kept, every day become less and less poisonous; and at last a more wholesome liquor. But in fact physicians find, that the colic of Poitou is not an uncommon disorder in London; and it is more than probable that
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the Devonshire cyder, whether unmixed with other liquors, or as it enters a composition, which is drunk for wine, may contribute towards making the disease so common as it is.

But now follows Mr. MORE's two letters, which, according to Mr. GEACH, prove that all the experiments depended upon a blunder. But how far these letters prove what Dr. HUXHAM and Mr. GEACH intend by the publication of them, I shall leave to the intelligent reader. I must however observe, that the following part of Mr. MORE's first letter is suppressed; "As it doth not appear by any thing mentioned in the Pamphlet, that the rest of the experiments were made on cyder different from that made use of in this experiment, it is not at all strange, that evident marks of its containing lead should appear, the whole question being, whether it became impregnated with metal in the bottles, or in the mill, or press, at the time of making." You informed me that a passage in *your Essay*, page 44, was pointed out to Mr. MORE, in which it is said, "*The same experiments were afterwards tried on several other specimens of*

Devonshire and Herefordshire cyder. The result of them was constantly and uniformly the same as has been described.' Mr. MORE was likewise informed, that the experiments had been frequently made on cyder, taken from the cask, as well as bottled cyder. Why this part of Mr. MORE's letter was suppressed, will be obvious to every one, who has been in the least conversant with that species of fallacy, in which, it must be confessed, Mr. GEACH has made a very contemptible figure.

About the month of October last, I received from you 18 quarts of cyder, from which I was to prepare an extract, and make an assay, not doubting but that the quantity of lead detected by you, in your former experiments, might be still rendered more sensible. I evaporated the liquor in a stone vessel, to the consistence of an extract, and was somewhat surprised to find in the extract a small quantity of malleable lead; this I immediately suspected might arise from shot in the bottles; of this suspicion you had the earliest intelligence. I picked out all the lead carefully; and my own private assaying furnace not being in order, I carried it to be assayed at a place

place where I met with Mr. MORE: I told what I was engaged in, and immediately hinted to him my suspicion regarding the accuracy of that experiment, and suggested the propriety, and urged the necessity of repeating it: however I proceeded in the assay, although I meant to lay no stress on it; this he knew perfectly well, as I told him, I would set about making another extract immediately: the result of the assay of which is what is referred to in your fifth experiment.

It may be perhaps of some service to Mr. MORE, if I publickly acknowledge that he used many arguments, to dissuade me from pursuing such an enquiry; and refused either to encourage, or even be witness to any experiments, which might end in discoveries in any way disagreeable to his very good friends of the county of Devon.

If the public, however, should still hesitate about the validity of this last assay, as being from bottled cyder, their doubts will be removed, when your papers are published, in which they will see, that an assay has been made, under the eye of several gentlemen of

candor and veracity, from cyder taken immediately from the cask, by which a quantity of malleable lead was obtained.

Mr. ALCOCK's remarks, are rather more sensibly written than the observations of Mr. GEACH; and I think they really deserve to have taken place of the other performance; and why Mr. GEACH, who asks permission to *prefix* them to his work, places them in the rear, except from a sense of his own superiority, I cannot conjecture. But these remarks are, according to Mr. ALCOCK, only *cursor*y; and verily a man who runs may answer them.

The first remark has already been considered in part. But I will inform this Reverend Author, that in medical matters, we pay adulation to no *ipse dixit*; nor do we allow that the authority of any man whatever, can stamp infallibility on opinions which are not established by experiment.

Remark 2. The gentleman is disposed to make himself merry on the subject of iron cramps. What is said of the Herefordshire troughs, stands on the authority of Dr. WALL

of Worcester, a gentleman whose learning and accuracy are well known. No other answer is necessary.

Remark 3. Alphington being a place famous for making good cyder, and being so very near to Exeter, it was most obvious to try your experiments on the cyder of that parish; but trials were made from various other specimens; and if even in London, various specimens of cyder, agreed in showing marks of a Saturnine solution, the fairness of a general conclusion must be allowed.

He now finds himself under the necessity of saying, that “ *Perhaps all the difference of colour might be owing to a greater degree of acidity, in the Devonshire, than in the Herefordshire cyder.*” This supposition is still in your favour; for the more acid there is in the cyder, the lighter the colour of the precipitate would be. But let me refer Mr. ALCOCK to page 27, where he will find it to be allowed by one whom Mr. GEACH calls a *chemist*, that the dark precipitation, occasioned by the *liquor probatorius*, is an undoubted proof of the presence of lead.

Remark

Remark 4. “ I always thought that putty had been made of white lead, oil, and whiting.”

Whatever this gentleman's thoughts may have been, he may be assured that the farmers in Worcestershire, put no white lead into their putty. But let me advise him to consult Chambers's Dictionary, and he will there find, contrary to what he always thought, that putty, in its popular sense, denotes a cineritious kind of paste, compounded of whiting and linseed oil, beaten together to the consistence of a tough dough. Mr. ALCOCK, I imagine, will require no farther solution of this difficulty. However, supposing there had been white lead in the composition of this putty, I will inform Mr. ALCOCK, that the cyder could not have touched it, while it had chalk or whiting in the composition to act upon.

Remark 4. You never said that cyder was in the condition of *sacharum saturni*, but that lead dissolved in cyder was in that state; and this is certainly as you represent it. But I am utterly at a loss to guess how Mr. ALCOCK can speak with such absolute certainty, of the quantity of lead used in pounds, which were
made

made seventy or eighty years ago, or how he can determine that not a single pound of lead has been consumed in such a number of years. Did he weigh the exact quantity seventy or eighty years ago? And has he lately weighed the remainder?

Remark 6. This is an insignificant remark, on a quotation from a French author. Let the gentleman however enquire at the excise office; and he will there be informed, that those whose business it is to handle the tea, which is always imported in lead, are very apt to become paralytic in their arms and hands.

Remark 7. All this reasoning is absolutely inconclusive, as will appear to any one, who will consider the different effects of all the nervous poisons on different constitutions. And let me ask Mr. ALCOCK, upon what other principle he conceives it to depend, that according to the argument of his friend Mr. GEACH, not one in a thousand (of those I presume who drink cyder) has the endemial colic, page 18.

The

The tartar, according to Mr. ALCOCK, in a moderate quantity is salutary, in excess deleterious. What! does the very same substance taken in large quantities, bring on those very effects, which when taken in small quantities it remedies. In my turn, *I should be glad of a solution of this.*

As to the vague reasoning concerning the moisture of the Devonshire atmosphere, it seems to deserve no consideration. Upon the whole, I believe it will appear to all impartial and disinterested readers, *that you have overthrown the hypothesis of others, and that your own opinion must stand, having a solid foundation in truth.*

S I R,

I am your most humble

and obedient Servant,

London,

Nov. 4. 1767:

WILLIAM SAUNDERS.



